A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1801.

ARTICLE I.

Outlines of Experiments and Inquiries respecting Sound and Light. By THOMAS YOUNG, *M.D. F.R.S.*

(*Continued from page* 78.)

IV. *Of the Velocity of Sound.*

IT has been demonstrated, by M. de la Grange and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with an uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as Newton has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

V. *Of sonorous Cavities.*

M. de la Grange has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with the same velocity with which they arrived at that obstacle. When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the second again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a second, as double the breadth of the passage is contained in 1130 feet; and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also, in a less degree, if the reflected pulse coincides with the next but one, the next but two, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determining the sound, in the other, increasing its strength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It must be confessed, that much remains to be done in explaining the precise manner in which the vibration of the air in an organ pipe is generated. M. Daniel Bernouilli has solved several difficult problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

VI. *Of the Divergence of Sound.*

It has been generally asserted, chiefly on the authority of Newton, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from considering the phænomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the place

place where his hearer ſtands: and I am aſſured by a very reſpectable Member of the Royal Society, that the report of a cannon appears many times louder to a perſon towards whom it is fired, than to one placed in a contrary direction. It muſt have occurred to every one's obſervation, that a ſound ſuch as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the houſe or other obſtacle no longer intervened; and it has been already remarked by Euler, on this head, that we are not acquainted with any ſubſtance perfectly impervious to ſound. Indeed, as M. Lambert has very truly aſſerted, the whole theory of the ſpeaking trumpet, ſupported as it is by practical experience, would fall to the ground, if it were demonſtrable that ſound ſpreads equally in every direction. In windy weather it may often be obſerved, that the ſound of a diſtant bell varies almoſt inſtantaneouſly in its ſtrength, ſo as to appear at leaſt twice as remote at one time as at another; an obſervation which has alſo occurred to another gentleman, who is uncommonly accurate in examining the phænomena of nature. Now, if ſound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent diſtance: but, on the ſuppoſition of a motion nearly rectillinear, it may eaſily happen that a ſlight change in the direction of the wind, may convey the ſound, either directly or after reflection, in very different degrees of ſtrength, to the ſame ſpot. From the experiments on the motion of a current of air, already related, it would be expected that a ſound, admitted at a conſiderable diſtance from its origin through an aperture, would proceed, with an almoſt imperceptible increaſe of divergence, in the ſame direction; for, the actual velocity of the particles of air, in the ſtrongeſt ſound, is incomparably leſs than that of the ſloweſt of the currents in the experiments related, where the beginning of the conical divergence took place at the greateſt diſtance. Dr. Matthew Young has objected, not without reaſon, to M. Hube, that the exiſtence of a condenſation will cauſe a divergence in ſound: but a much greater degree of condenſation muſt have exiſted in the currents deſcribed than in any ſound. There is indeed one difference between a ſtream of air and a ſound; that, in ſound, the motions of different particles of air are not ſynchronous: but it is not demonſtrable that this circumſtance would affect the divergency of the motion, except at the inſtant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increaſe of intenſity. The ſubject, however, deſerves a more particular inveſtigation; and, in order to obtain a more ſolid foundation for the argument, it is propoſed, as ſoon as circumſtances permit, to inſtitute a courſe of experiments for aſcertaining, as accurately as poſſible, the different ſtrength of a ſound once projected in a given direction, at different diſtances from the axis of its motion.

VII. *Of the Decay of Sound.*

Various opinions have been entertained reſpecting the decay of ſound. M. de la Grange has publiſhed a calculation, by which its force is ſhown to decay nearly in the ſimple ratio of the diſtances; and M. Daniel Bernouilli's equations for the ſounds of conical pipes lead to a ſimilar

a ſimilar concluſion. The ſame inference would follow from a completion of the reaſoning of Dr. Helſham, Dr. Matthew Young, and Profeſſor Venturi. It has been very elegantly demonſtrated by Maclaurin, and may alſo be proved in a much more ſimple manner, that when motion is communicated through a ſeries of elaſtic bodies increaſing in magnitude, if the number of bodies be ſuppoſed infinitely great, and their difference infinitely ſmall, the motion of the laſt will be to that of the firſt in the ſubduplicate ratio of their reſpective magnitudes; and, ſince in the caſe of concentric ſpherical laminæ of air, the bulk increaſes in the duplicate ratio of the diſtance, the motion will in this caſe be directly, and the velocity inverſely, as the diſtance. But, however true this may be of the firſt impulſe, it will appear, by purſuing the calculation a little further, that every one of the elaſtic bodies, except the laſt, receives an impulſe in a retrograde direction, which ultimately impedes the effect of the ſucceeding impulſe, as much as a ſimilar cauſe promoted that of the preceding one: and thus, as ſound muſt be conceived to conſiſt of an infinite number of impulſes, the motion of the laſt lamina will be preciſely equal to that of the firſt; and, as far as this mode of reaſoning goes, ſound muſt decay in the duplicate ratio of the diſtance. Hence it appears, that the propoſal for adopting the logarithmic curve for the form of the ſpeaking trumpet, was founded on fallacious reaſoning. The calculation of M. de la Grange is left for future examination; and it is intended, in the mean time, to attempt to aſcertain the decay of ſound as nearly as poſſible by experiment: ſhould the reſult favour the concluſions from that calculation, it would eſtabliſh a marked difference between the propagation of ſound and of light.

VIII. *Of the harmonic Sounds of Pipes.*

In order to aſcertain the velocity with which organ pipes of different lengths require to be ſupplied with air, according to the various appropriate ſounds which they produce, a ſet of experiments was made, with the ſame mouth-piece, on pipes of the ſame bore, and of different lengths, both ſtopped and open. The general reſult was, that a ſimilar blaſt produced as nearly the ſame ſound as the length of the pipes would permit; or at leaſt that the exceptions, though very numerous, lay equally on each ſide of this concluſion. The particular reſults are expreſſed in Table XI. and in Plate V. Fig. 28. They explain how a note may be made much louder on a wind inſtrument by a ſwell, than it can poſſibly be by a ſudden impreſſion of the blaſt. It is propoſed, at a future time, to aſcertain by experiment, the actual compreſſion of the air within the pipe under different circumſtances: from ſome very ſlight trials, it ſeemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table

Table XI.

OPEN.

A	B	C	D	E	F
4.5		0.7	8.8	$\overline{\overline{\overline{d}}}$*	1
	4.1	8.8			2
9.4		0.3	0.9	$\overline{\overline{f}}$	1
	0.8		8.0		2
	2.0		18.0		3
	5.0	8.0	20.0		4
	16.5	18.0			5
	19.0	20.0			6
16.1		0.4	1.0	$\overline{\overline{g}}$*	2
	0.8	1.0	2.2		3
	1.2	2.2	4.7		4
	2.2	4.7	11.5		5
	3.4		13.5		6
	4.0		15.0		7
	6.5	10.0			8
20.5		0.6	0.8	$\overline{\overline{b}}$	3
		0.8	1.9		4
	1.1	1.9	5.7		5
	4.5	5.7			8

STOPPED.

A	B.	C	D	E	F
4.5		0.3	1.8	$\overline{\overline{d}}$	1
	1.2	1.7	10.0		3
	5.0	9.0			5
9.4		0.2	0.4	$\overline{f}$	1
		0.45	1.6		3
	1.1	1.6	8.5		5
	7.0	8.0			7
16.1		0.4	0.6	$\overline{\overline{d}}$*	3
	0.6	0.65	1.1		5
	0.9	1.1	2.4		7
	1.6	2.4	4.9		9
	2.5	4.8	9.0		11
	6.0	7.0			13
20.5		0.8	1.1	$\overline{\overline{\overline{c}}}$*	7
	1.0	1.1	3.8		9
	1.8		3.8		11
	3.2	3.8	12.		17
		12.		o	00

A, is the length of the pipe from the lateral orifice to the end. C, the preſſure at which the ſound began. B, its termination, by leſſening the preſſure; D, by increaſing it. E, the note anſwering to the firſt ſound of each pipe, according to the German method of notation. F, the number ſhowing the place of each note in the regular ſeries of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece meaſured, where ſmalleſt, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a preſſure of above 22 inches. Where no number ſtands under C, a ſudden blaſt was required to produce the note.

IX. *Of the Vibrations of different elaſtic Fluids.*

All the methods of finding the velocity of ſound, agree in determining it to be, in fluids of a given elaſticity, reciprocally in the ſubduplicate ratio of the denſity: hence, in pure hydrogen gas it ſhould be $\sqrt{13}=3.6$ times as great as in common air; and the pitch of a pipe ſhould be a minor fourteenth higher in this fluid than in the common air. It is therefore

therefore probable that the hydrogen gas used in Professor Chladni's late experiments, was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French Academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence, the velocity was found to be only 1109 feet in a second; while Derham's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may here be remarked, that the well known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. Smith has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. Bianconi found the velocity of sound, at Bologna, to differ at different times, in the ratio of 152 to 157.

X. *Of the Analogy between Light and Sound.*

Ever since the publication of Sir Isaac Newton's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. Leonard Euler indeed, in several of his works, has advanced some powerful objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration, which after Huygens and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the Huygenian. There are also one or two difficulties in the Newtonian system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of two pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should be always reflected, and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phænomena of

 electricity;

electricity; and the arguments against the existence of such an ether throughout the universe, have been pretty sufficiently answered by Euler. The rapid transmission of the electrical shock, shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the Newtonian theory, favours the admission of the Huygenian; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown, that sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small, and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of Newton and of Mr. Jordan, was not partly occasioned by the tendency of light to diverge. Euler's hypothesis, of the transmission of light by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought to be a million times greater than it is. For explaining the phænomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as Euler supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum. Let AB, Plate V. Fig. 29, be a ray of light falling on the reflecting surface FG; *c d* the direction of the vibration, pulse, impression, or condensation,

fation. When *d* comes to H, the impreffion will be, either wholly or partly, reflected with the fame velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, Fig. 30, be a refracting furface. The portion of the pulfe IE, which is travelling through the refracting medium, will move with a greater or lefs velocity in the fubduplicate ratio of the denfities, and HE will be to KI in that ratio. But HE is, to the radius IH, the fine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the fame as that of Euler. The total reflection of a ray of light by a refracting furface, is explicable in the fame manner as its fimple refraction; HE, Fig. 31, being fo much longer than KI, that the ray firft becomes parallel to FG, and then, having to return through an equal diverfity of media, is reflected in an equal angle. When a ray of light paffes near an inflecting body, furrounded, as all bodies are fuppofed to be, with an atmofphere of ether denfer than the ether of the ambient air, the part of the ray neareft the body is retarded, and of courfe the whole ray inflected towards the body, Fig. 32. The repulfion of inflected rays has been very ably controverted by Mr. Jordan, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by Euler, that the colours of light confift in the different frequency of the vibrations of the luminous ether: it does not appear that he has fupported this opinion by any argument; but it is ftrongly confirmed, by the analogy between the colours of a thin plate and the founds of a feries of organ pipes. The phænomena of the colours of thin plates require, in the Newtonian fyftem, a very complicated fuppofition, of an ether, anticipating by its motion the velocity of the corpufcles of light, and thus producing the fits of tranfmiffion and reflection; and even this fuppofition does not much affift the explanation. It appears, from the accurate analyfis of the phænomena which Newton has given, and which has by no means been fuperfeded by any later obfervations, that the fame colour recurs whenever the thicknefs anfwers to the terms of an arithmetical progreffion. Now this is precifely fimilar to the production of the fame found, by means of an uniform blaft, from organ-pipes which are different multiples of the fame length. Suppofing white light to be a continued impulfe or ftream of luminous ether, it may be conceived to act on the plates as a blaft of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting furfaces. It may be objected that, to complete the analogy, there fhould be tubes, to anfwer to the organ-pipes: but the tube of an organ-pipe is only neceffary to prevent the divergence of the impreffion, and in light there is little or no tendency to diverge; and indeed, in the cafe of a refonant paffage, the air is not prevented from becoming fonorous by the liberty of lateral motion. It would feem, that the determination of a portion of the track of a ray of light through any homogeneous ftratum of ether, is fufficient to eftablifh a length as a bafis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmofphere may determine its vibrations: but, in this cafe, as it is probable that there is a reflection from every part of the furface of the furrounding atmofphere, con-

3 tributing

tributing to the appearance of the white line in every direction, in the experiments already mentioned, so it is possible that there may be some second reflection at the immediate surface of the body itself, and that, by mutual reflections between these two surfaces, something like the anguiform motion suspected by Newton may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction: yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count Rumford, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

XI. *Of the Coalescence of Musical Sounds.*

It is surprising that so great a mathematician as Dr. Smith could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phænomena of beats, and of the grave harmonics observed by Romieu and Tartini; which M. de la Grange has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and, in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the abscis, and the distance of the particle from its original position, by the ordinate, Fig. 33—38. Then, by supposing any two or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When two sounds are of equal strength, and nearly of the same pitch, as in Fig. 36, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, Plate VI. Fig. 43, B and C: which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in

exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In Plate V. Fig. 33, let P and Q be the middle points of the progress or regress of a particle in two successive compound vibrations; then, CP being = PD, KR = RN, GQ = QH, and MS = SO, twice their distance, 2 RS = 2 RN + 2 NM + 2 MS = KN + NM + NM + MO = KM + NO, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the two sounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of two sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini. For instance, in Plate V. Fig. 34—37, the vibrations of sounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between two successive coincidences, or near approaches to coincidence: for, that such a tempered interval still produces a harmonic, appears from Plate V. Fig. 38. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{5}{15}$, $\frac{4}{12}$, or $\frac{3}{9}$ of the whole period: hence, in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave, as is seen in some degree in Plate V. Fig. 35; AB being nearly two-thirds of CD. The same sound is sometimes produced by taking the minor sixth below the key note; probably because this sixth, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all: if, however, the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked Plate VI. Figs. 41, 42: the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency as the series of odd numbers and in strength inversely in the same ratios, the right lines indicating an uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in Plate V. Figs. 39, 40.

VII. *Of*

XII. *Of the Frequency of Vibrations constituting a given Note.*

The number of vibrations performed by a given sound in a second, has been variously ascertained; first, by Sauveur, by a very ingenious inference from the beats of two sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by Mersennus, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at one-eighth of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be one-sixth of a tone higher than the respective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch assumed by Sauveur, of an imaginary C, consisting of one vibration in a second.

(To be concluded in our next.)

II.

On the Identity of the Pyro-mucous, Pyro-tartareous, and Pyro-ligneous Acids, with the Acetous Acid. By Citizens FOURCROY *and* VAUQUELIN *.

THE re-union of species which have been erroneously considered as different, is in many instances of greater advantage to the science of facts, than the discovery of new species. It is towards this degree of perfection that the efforts of chemists and naturalists are at present directed.

Three acids have been distinguished by different appellations, in which the common property of being produced by the action of fire, and of acquiring several common characters from that agent, is well known; but it was never imagined that they are not only identically the same, but likewise analogous in their nature to the acid of vinegar, denominated acetous acid. Citizens Fourcroy and Vauquelin, in their endeavours to discover certain characters by which the pyro-mucous, pyro-tartareous, and pyro-ligneous acids might be distinguished, were unable to discover any. By comparing those which have hitherto been pointed out as the distinctive characters, it may be shewn, as they have done, that they are founded merely on slight differences of smell and colour; and are consequently delusive.

* Bulletin de la Soc. Philom.---N.

Some

Some facts obferved in the analyfes of vegetables, led our chemifts to fufpect that thefe acids, which were proved to be the fame, might not differ from the acetous acid, except by foreign additions. They made experiments with a view to confirm their fufpicions. By the diftillation of fugar, ftarch, gum, &c. they obtained the pyro-mucous acid; by that of tartar, the pyro-tartareous acid; and by that of wood, the pyro-ligneous acid. They combined thefe acids with an alkaline bafis; from which, having feparated them by the fulphuric acid, they obtained by this means, in fome inftances, with filtration through charcoal powder, an acid and limpid liquor, deprived of the empyreumatic oil, which rendered it impure, and prevented it from being diftinguifhed as the true acetous acid. This liquor then poffeffed the fmell, tafte, and all the other characters; and, with the alkaline bafes, formed perfect acetites.

It was neceffary to attempt to form the empyreumatic acids again by the acetous acid, by imparting to the latter the empyreumatic oil and empyreumatic fmell, of which it had been deprived by the preceding experiments. The following is the procefs employed by Citizens Fourcroy and Vauquelin:—They diftilled a portion of acetous acid over the empyreumatic oils of mucilage, tartar, and wood; and thus formed pyro-mucous, pyro-tartareous, and pyro-ligneous acids. To form thefe acids again inftantaneoufly, it was only requifite to add a few drops of thofe empyreumatic oils to fome acetous acid.

To thefe facts, which are fufficiently interefting in themfelves, the difcoverers have fubjoined reflexions no lefs important, relative to the frequent production of the acetous acid under very different circumftances.

The acetous acid is not therefore a neceffary product of the vinous fermentation. In addition to the facts above related, which confirm the production of this acid without any fermentation, it may be obferved, that many fubftances, of a nature very different from wine, contain acetites; fuch are the fap of vegetables kept only a few hours, foils, heated tan, the four water of the ftarch matter, four pulfe, and four fruit, expofed for fome hours to a hot air. Milk, animal jellies, and that part of urine called *urée*, when they become four, likewife afford a great quantity of this acid.

Citizens Fourcroy and Vauquelin have difcovered four very diftinct circumftances under which the acetous acid is produced, each of which is characterifed by fome particulars relative to the phenomena of its production.

The firft mode of *acetification* is the decompofing action of fire: in this cafe water is difengaged with carbonic acid formed from the difengaged carbon; and the acetous acid thus obtained is diftinguifhed by its colour and empyreumatic odour.

The fecond method confifts in the application of the powerful acids, fuch as the fulphuric, nitric, and the oxigenated muriatic acids upon vegetable compounds, fuch as fugar, jelly, &c.; alcohol and the vegetable acids themfelves, except the acetous acid, are entirely decompofed by thefe acids. Some water and carbonic acid are alfo produced, and the acetous acid thus obtained contains oxalic acid, malic acid, and much water.

The

The third and most general method is the fermentation of wine. In this process neither water nor carbonic acid is produced; but an absorption of oxygen takes place: this acetous acid contains tartar, alcohol, and colouring matter.

The fourth mode, which is nearly similar to the third, the difference, perhaps, being only because it is no longer the vegetable fluid called *wine* which produces the acetous acid, is a particular fermentation which takes place in certain animal or vegetable products above-mentioned, but especially in urines. This acid is always combined with ammonia.

III.

On the Structure and Uses of the Membrana Tympani of the Ear. By EVERARD HOME, *Esq. F. R. S.* *

THE subject of inquiry appointed by the Croonian Institution, has been greatly elucidated at different times by ingenious members of this learned Society. A large field, however, still remained open; and, respecting future investigations, I shall have occasion to offer a fresh proof of the aid to be derived from comparative anatomy, in ascertaining the structure of parts which, from their minuteness and situation in the human body admit with much difficulty of being explored.

The principal object of the present lecture is to communicate a discovery of the structure of the membrana tympani; which, in some respects, affords a new and very curious instance of the application of muscular action, and may conduce to account for certain phænomena in the sense of hearing, in a more satisfactory manner than has hitherto been proposed.

The membrana tympani has always been considered as a common membrane, which, by means of muscles belonging to the malleus being stretched or relaxed, became fitted, in its various degrees of tension, to convey the vast variety of external sounds to the internal organ. Its shape, situation, and office, have procured it the name of drum of the ear; and the muscles of the malleus having been deemed sufficient for bracing and unbracing it, less attention was bestowed on the structure of the membrane itself: to which may be added, that in the human ear, and generally in the ear of quadrupeds, the membrane is so extremely small and thin, and in its situation so peculiarly confined, as not to be got at for inspection but with much difficulty.

The case is different in the elephant, where this membrane is so very large, that the parts of which it is composed are readily distinguished: they are even conspicuous to the naked eye; and muscular fibres are seen passing along the membrane, in a radiated manner, from the bony rim which surrounds it, towards the handle of the malleus, to which the central part of the membrane is firmly attached.

* Philos. Transactions, 1800.

This diſcovery in the elephant having led to that of a ſimilar conſtruction in the human membrana tympani, it may not be improper to relate the circumſtances by which I became engaged in the inveſtigation of the organ of hearing in that animal.

Three different opportunities have occurred of diſſecting the elephant in London, by the deaths of thoſe which had been preſented to his Majeſty, and were kept at the King's ſtables at Pimlico. One of them was given to the late Dr. Hunter; one to his brother Mr. J. Hunter; and the third to Sir Aſhton Lever.

From my being connected with Mr. John Hunter's purſuits in comparative anatomy, I was employed throughout the whole of theſe diſſections, and became extremely deſirous of examining the internal parts of the ear, the ſtructure of that organ in the human body having at a very early period particularly engaged my attention;* but neither Dr. Hunter nor his brother could be prevailed upon to ſacrifice ſo large a portion of the ſkull as was neceſſary for the purpoſe.

When Mr. Corſe arrived from Bengal, laſt year, and mentioned his having brought over a number of ſkulls of elephants, in order to ſhow the progreſs of the formation of their grinding teeth,† the deſire to examine the organ of hearing in that animal recurred to me ſo ſtrongly, that I requeſted to have one of the ſkulls for that purpoſe, and Mr. Corſe very readily and obligingly complied with my requeſt.

After having examined the organ in the dried ſkull, the want of the membrana tympani, and of the ſmall bones, made the information thus received of a very unſatisfactory nature, and increaſed the deſire of ſeeing theſe parts in the recent head. In conſidering how this could be done, I recollected a mutilated elephant's head, preſerved in ſpirits, which had been ſent to Mr. Hunter, but, from the multiplicity of his engagements, had remained neglected in the caſk at the time of his death, and in the following year was dried, to ſhow the proboſcis, that it might not be altogether ſpoiled.

Upon examining this dried head, the bones had been ſo much broken, that one of the organs of hearing was altogether wanting: the other, however, was fortunately entire; and the membrana tympani and ſmall bones, having been little diſturbed in the drying of the parts, remained nearly in their natural ſituation.

* In the year 1776, I injected the cochlea and ſemicircular canals of the human ear with a compoſition of wax and roſin. This was done by placing the temporal bone in the receiver of an air pump, the upper part of which was in the form of a funnel, rendered air-tight by a cork being fitted into its neck, and ſurrounded with bees' wax. After the air had been exhauſted, the hot injection, poured into the funnel, melted the wax, and the cork was pulled out by means of a ſtring previouſly attached to it; the injection immediately ruſhed into the receiver, and was forced, by the preſſure of the atmoſphere, into the cavities of the temporal bone.

† On this ſubject, a very ingenious paper has been ſince publiſhed by him, in the Philoſophical Tranſactions for the year 1799.

"The

The membrana tympani, and every other part of the organ, were found to be much larger in proportion than in other quadrupeds, or in man; differing in this reſpect from the eye of the elephant, which is unuſually ſmall, when compared with the enormous bulk of the animal.

The membrane was found of an oval form; the ſhort diameter of the oval rather more than an inch in length; the long diameter an inch and $\frac{7}{20}$ths.

In the human ear, the membrana tympani is nearly circular; the longeſt diameter is $\frac{8}{20}$ths of an inch; the ſhorteſt $\frac{7}{20}$ths.

As the membrane in the elephant exceeds that of the human ear in thickneſs as much as in extent, which is as the ſquares of their diameters, or in the proportion of 135 to 14, it is natural to conclude that the muſcular fibres which are to ſtretch the one, muſt greatly exceed in ſtrength thoſe capable of producing the ſame degree of tenſion in the other.

From this ſtatement, the muſcular ſtructure in the human membrana tympani will neceſſarily be ſo much leſs diſtinct than in the elephant, as ſcarcely to be viſible to the naked eye, and will eaſily be overlooked by the moſt attentive obſerver, who is not directed by ſome previous information to examine it under the moſt favourable circumſtances; but, when theſe are attended to, it can be perceived without the aid of glaſſes.

If the membrana tympani of the human ear is completely expoſed on both ſides, by removing the contiguous parts, and the circular covering is carefully waſhed off from its external ſurface, then, by placing it in a clear light, the radiated direction of its fibres may be eaſily detected. If a common magnifying glaſs is uſed, they are rendered nearly as diſtinct as thoſe of the elephant appear to the naked eye; their courſe is exactly the ſame; and they differ in nothing but in being formed upon a ſmaller ſcale.

When viewed in a microſcope magnifying 23 times, the muſcular fibres are beautifully conſpicuous, and appear uniformly the ſame throughout the whole ſurface, there being no central tendons, as in the diaphragm; the muſcular fibres appear only to form the internal layer of the membrane, and are moſt diſtinctly ſeen when viewed on that ſide.

In examining this membrane in different ſubject, the parts were frequently found in a more or leſs morbid ſtate. In one inſtance, the membrane was found loaded with blood-veſſels, was leſs tranſparent than uſual, and was united by cloſe adheſion to the point of the long proceſs of the incus. In another inſtance, there was a preternatual oſſification adhering to it, at a ſmall diſtance from the end of the handle of the malleus.

As muſcles in general are ſupplied with blood-veſſels in proportion to the frequency of their action, it is an object of importance to determine the vaſcularity of the membrana tympani. Upon this ſubject, my own want of information has been amply ſupplied by Dr. Baillie, who, in a communication upon this ſubject, ſhowed me a preparation of the membrane, in which the veſſels had been moſt ſucceſsfully injected with coloured wax.

In this preparation, the moſt beautiful of the kind I ever ſaw, the veſſels in their diſtribution reſembled thoſe of the iris, and were nearly half as numerous: they anaſtomoſed with one another in a ſimilar manner, and their general direction was from the

circumference to the handle of the malleus; from near this handle, a ſmall trunk ſent off branches, in a radiated manner, which anaſtomoſed with thoſe which had an oppoſite courſe.

This correſpondence, in the number and diſtribution of blood veſſels, between the membrana tympani and the iris, is a ſtrong circumſtance in confirmation of that membrane being endowed with muſcular action.

In the horſe, the membrana tympani is ſmaller than in man; its long diameter is $\frac{8}{20}$ths of an inch; the ſhort one $\frac{6}{20}$ths; and it is almoſt quite flat, while in man it is concave. which makes the difference of extent conſiderably exceed the difference in the diameters, In the horſe, the fibrous ſtructure is not viſible to the naked eye; it is even indiſtinctly ſeen when viewed through a common magnifying glaſs; but in a microſcope it is very viſible, and in every other reſpect agrees in ſtructure with the membrane in the human ear, and in that of the elephant.

In birds, the membrana tympani is larger in proportion than in the quadruped, and more circular in its ſhape.

In the gooſe, it is $\frac{6}{20}$ths of an inch in its longeſt diameter, and $\frac{5}{20}$ths in its ſhorteſt diameter. In the turkey, $\frac{7}{20}$ths by $\frac{5}{20}$ths. It is thinner in its coats in birds than in the horſe, and to the naked eye has no appearance of fibres; but, when viewed in a microſcope, there is a viſible radiated ſtructure, not very unlike the wire marks upon common writing paper.

In a former Lecture upon the Structure of Muſcles,* in which a comprehenſive view was taken of the ſubject, it was ſtated, that the organization neceſſary for muſcular contraction could exiſt in an apparent membrane, and that a faſciculated ſtructure was only neceſſary when muſcular action was to be enabled to overcome reſiſtance. The coats of the Tænia hydatigena were mentioned as an inſtance of the firſt; and the human heart as the moſt complex of the ſecond. In comparing the membranæ tympani of different animals, they afford a beautiful illuſtration of the truth of this poſition.

In birds, where from the ſmallneſs of its ſize the reſiſtance is very trifling, the membrane is very ſimilar to the coat of an hydatid, only ſtill thinner. In the elephant, fibres forming faſciculi are very diſtinct. The membrane of the horſe, and that of the human ear, form the intermediate gradations.

The knowledge of a muſcular ſtructure in the membrana tympani, enables us to explain many phænomena in hearing, which have not hitherto been accounted for in a ſatisfactory manner. It is principally by means of this muſcle that accurate perceptions of ſound are communicated to the internal organ, and that the membrana tympani is enabled to vary the ſtate of its tenſion, ſo as to receive them in the quick ſucceſſion in which they are conveyed to it.

* Philoſophical Tranſactions for the year 1795.

In

In the human ear, and in that of birds, the radiated fibres of the membrana tympani have their principal attachment to the extremity of the handle of the malleus, which is nearly in the centre of the membrane.

In the membrane of the elephant, which is oval, the attachment to the handle of the malleus is at ſome diſtance from the centre. In the horſe, deer, and cat, which have the membrane ſtill more oval than the elephant, the handle of the malleus is ſituated in the long axis of the membrane, with its extremity extending beyond the centre, reaching nearer to the circumference; and the fibres of the radiated muſcle are not only attached to its end, but alſo laterally to nearly the whole length of its handle.

This oval form of the membrana tympani, in thoſe quadrupeds, and the very extenſive attachment of the fibres of the radiated muſcle to the handle of the malleus, may be the reaſon why their ears are not equally fitted to hear inarticulate ſounds, as the ears of birds and of man.

Should this radiated muſcle of the membrana tympani (which is probably the ſmalleſt in the body that has a diſtinct action) be thought too inſignificant to have an office of ſo much conſequence aſſigned to it, let it be remembered, that the ſize of muſcles is no indication of their importance, but only of the reſiſtance to be overcome by their action; and that the more delicate actions are performed univerſally in the body by very ſmall muſcles, of which the iris in the eye furniſhes a very conſpicuous example.

(*To be continued.*)

IV.

Inquiries relative to the Laws of Affinity, by CIT. BERTHOLLET, *Member of the National Inſtitute of Arts and Sciences*.*

(*Concluded from page 69.*)

ARTICLE XIII.

On the Precipitation of Metallic Solutions by other Metals.

1. WHEN the metals are precipitated by a ſubſtance which does not ſeize their oxigen, the precipitates retain part of the acid, and often part of the precipitant.

We have a ſtriking inſtance of this diviſion, which then takes place in the precipitate of the oxigenated muriate of mercury, by the fixed alkalis, ammonia and lime. On expoſing the precipitate to a ſufficient heat, a more or leſs conſiderable portion of mercury, according to the nature of the precipitate, is reduced, another is ſublimed, and forms a muriate, not becauſe the muriatic acid is found combined only with a portion of the oxide of mer-

* Tranſlation from the Annales de Chimie, XXXVII. 151.

cury, as I ſuppoſed, (*Memoirs of the Academy*, 1780) but becauſe the expanſive force of the heat, and the tendency to the combination of the muriatic acid, acting upon the oxide of mercury, cauſe, as we may expreſs ourſelves, a new ſharing to be made of it. If the precipitate by ammonia be examined, it is obſerved, that it retains a portion of ammonia: the precipitate of the muriate of iron by pot-aſh, retains part of the pot-aſh. Facts of this kind may be collected in great number.

There is therefore no doubt, but that the obſervations which have been made upon the precipitations of ſubſtances which loſe their ſolubility, cannot be applied to the metallic precipitates, which vary according to all the circumſtances which are capable of modifying the powers brought in action, at the moment of their ſucceſſive precipitation, and which will deſerve a particular examination in another memoir.

But when the metals are mutually precipitated from their ſolutions, their reciprocal affinity for oxigen has a great effect in the action that takes place; and ſometimes the precipitate is found in the metallic ſtate. If another force were not joined to the affinity of the precipitating metal for oxigen, it would naturally follow from the principles eſtabliſhed in this Memoir, that the oxigen would always be divided between the two metals which are in competition, according to the action they exert upon it. It is therefore neceſſary to examine, what may be the force which determines the precipitation in the metallic ſtate?

2. The affinity of mercury, gold, and ſilver for oxigen is very weak: the mutual affinity which ſtill remains between the parts of theſe metals when they are in fuſion, as mercury is at the temperature of the atmoſphere, is ſufficient to prevent their combination with oxigen in the ſtate of gas; but heat, by dilating the particles of the mercury, ſufficiently diminiſhes the force of their mutual affinity to enable them to combine with oxigen: a ſuperior degree of heat is ſufficient, by the difference of dilatation which it produces in the mercury and in the oxigen, to ſeparate them: ſo that the action of the heat begins by being more effective to this combination, in conſequence of the diminution of the mutual affinity of the parts, than inimical to it by the different dilatation of the mercury and the oxigen: but at length by augmenting this difference, it renders their combination impoſſible.

As the force of coheſion in mercury is ſufficient to prevent oxidation, this very force will tend to effect its diſoxigenation, with the aſſiſtance of a metal which will act directly upon the oxigen. It is a force analogous to that which produces cryſtalliſation and precipitations. (Art. V.)

3. The metallic particles have not only a mutual affinity, but likewiſe have an affinity for thoſe of other metals: hence proceed the amalgams and mixtures. It is only neceſſary to put copper in contact with mercury to effect a combination between theſe two metals. Thus when a metal oppoſes its action to a metallic ſolution, part of the metal may act upon the oxigen and upon the acid, while the other tends to combine with the metal of the oxide. Let us examine, whether we can diſcover in the precipitation of mercury, ſilver

gold

gold and copper, in the metallic ſtate, the influence of theſe two forces, that is to ſay, the mutual affinity of the particles of one and the ſame metal, and the affinity of one metal for another.

4. When a piece of copper is plunged in a ſolution of mercury by the nitric acid, or by the muriatic acid, the copper becomes inſtantly white, and the mercury is found to be reduced; but it has combined with the copper.

If inſtead of a plate of copper, a plate of very clean iron be plunged in the ſame ſolution, ſeveral hours elapſe before the liquid appears diſturbed, and a precipitate is perceived: at length a precipitate is formed; but with the muriatic ſolution in particular, this precipitate is partly in the ſtate of an oxide, and very probably retains a portion of acid.

If the affinity of a metal for oxigen were the only cauſe which produced the precipitation of another metal, the iron ought to act with much greater efficacy than the copper; for it is known to have a much ſtronger affinity for oxigen, and neverthelefs its action is ſlow, difficult and incomplete, while that of copper is inſtantaneous. It is ſeen by the manner in which the indecompoſable acids are retained by the oxide of copper and the oxide of iron, when their combination is expoſed to the action of heat, that there can only be a very ſmall difference between the affinities of theſe metals for the acids. There is conſequently no doubt that the affinity of the copper for the mercury, with which it has actually combined, muſt have greatly contributed to its precipitation in the metallic ſtate; but the mutual affinity of the particles of mercury, muſt alone have decided, though with difficulty, in the experiment with iron, the reduction of the former metal; a portion was alſo precipitated in oxide, and probably retained a portion of acid, as the whole would have done, if the affinity of the iron for oxigen had been the ſole agent; and the portion precipitated in the metallic ſtate did not combine with the iron.

5. When a ſolution of ſilver is precipitated by copper, the precipitate which is found in the metallic ſtate is not of pure ſilver, but a combination of ſilver with a ſmall portion of copper: it could not take the copper from the plate itſelf which was plunged in the ſolution; it muſt therefore have precipitated with it out of the ſolution: the mutual affinity of theſe two metals has decided their diſoxigenation. By means of this force two combinations are effected, as happens alſo in ſeveral other circumſtances: one of the acids, with the oxide of copper, the other of ſilver with a portion of the copper. The action of the acid upon the oxide of copper, and that of ſilver upon copper, are thus put in equilibrio.

6. So likewiſe when a plate of copper is plunged into a ſolution of gold, the gold which is precipitated ſhews by its higher colour, that it has combined with copper, and the ſolution retains but a ſmall part of the copper which has been loſt by the plate.

If a plate of iron be put in this ſolution, the gold which is precipitated ſeizes, perhaps equally, a part of the iron, or at leaſt its precipitation is determined by the affinity of the iron for the gold, at the ſurface of which this laſt combines. For the gilding is a combination of the two metals at the ſurface, by which they are in contact: when the firſt ſtratum

is formed, the precipitation is capable of continuing by the mere force of cohesion of the gold to itself.

7. What I have just stated is verified in the precipitation of copper by iron. When a solution of copper is decomposed by a plate of iron, and the copper attached to the plate is separated, it is perceived by the brown colour of the interior surface, that the copper is not there pure, and that it contains some iron. After this first stratum the copper continued to precipitate, by contracting an adherence with itself, and afterwards with those coats which are successively formed; so that the affinity for the iron began the effect, which was continued by that of the copper for itself.

8. Phosphorus precipitates several metallic solutions, as has been explained by Sage and Bouillon *. Though it has a strong affinity with oxigen, yet what has just been explained relative to precipitation by the metals, ought to be applied to its action.

Pelletier has proved, that phosphorus has the property of combining with metals, so that a part of that which is put in action is capable of combining with oxigen, while the other acts equally upon the metal.

Among the metallic solutions there are some which are not affected by phosphorus; in others the metal is precipitated in oxide, which doubtless retains part of the acid of the solution, or of the phosphoric acid which is formed; in others the metal is at length reduced. Gold, silver, copper, and mercury, are among those which resume the metallic state.

On considering the observations which have been made on this precipitation, it appears that copper and silver are precipitated by combining with a small portion of phosphorus; thus, to precipitate twelve grains of silver, three grains of phosphorus are consumed. But only about three grains of phosphoric acid, in a gelatinous state, were obtained: now, one grain of phosphorus only would be necessary to produce this quantity of acid; there consequently remain more than two grains, which must have combined with the silver.

A part only of mercury thus treated resumes the metallic state; the rest preserves the state of an oxide, and combines with the phosphoric acid. Here the force of cohesion is weak; and no combination is made with the phosphorus: hence the effect is but partial, as when iron is used. (No. IV.)

Though the affinity of gold for oxigen is very weak, and much inferior to that of copper, its precipitation is not so quickly effected, and a portion is precipitated in the state of oxide; this doubtless happens because it has little disposition to combine with phosphorus; and it is by the combination of copper with phosphorus, that the precipitation of the latter metal is determined.

Some of the observations which I have here presented require the accuracy of experiment for their entire explanation: but all appear to me indubitably to prove, that it is the force of cohesion which tends to recombine the particles of a one individual metal, and it is the

* Journal de Physique, 1781.

mutual

mutual affinity of ſome metals which decides their precipitation in the metallic ſtate; ſo that this ſtate is more or leſs ſudden and complete, according to the energy with which theſe cauſes are capable of acting *.

ARTICLE XIV.

Of the reſulting Affinity.

1. I denominate that the *reſulting affinity*, whoſe action proceeds from ſeveral affinities in the ſame ſubſtance: for inſtance, the nitric acid is compoſed of oxigen and azote; this acid combines with pot-aſh; it acts upon pot-aſh by an affinity which reſults from that of the oxigen and that of the azote. The reciprocal mutual action of the pot-aſh is likewiſe a force which reſults from that exerted by it upon each of the ſubſtances which compoſe the nitric acid.

2. All bodies exiſting upon the earth have an affinity for each other. If we refuſe to admit this principle, it muſt be allowed, that the number of exceptions can be but very ſmall. I may therefore reaſon upon this ſuppoſition, and apply to all ſubſtances that which obſervation has made known relative to the affinities and their modifications. If this application be not forced, if it accounts for properties which cannot be directly eſtabliſhed upon experiment, the conſiderations which I preſent in this article may throw ſome light upon ſeveral phenomena, which are owing to a chemical action ſtill undetermined.

3. I have ſuppoſed in the definition of *reſulting affinity*, that the affinity of a compound ſubſtance is derived from thoſe of the ſubſtances which compoſe it. It is neceſſary to examine, what are the circumſtances which might modify the elementary affinities, and aſcertain the changes which muſt have ariſen in the affinity reſulting from them.

4. The chemical action of ſubſtances is weakened in proportion to their ſaturation. (Article II. No. 10.)

It muſt be concluded, therefore, that the reſulting affinity muſt be a leſs quantity than the elementary affinities when alone: for the latter have experienced a commencement of ſaturation; but other circumſtances may increaſe the action of the reſulting affinity, or may augment the weakneſs it derives from ſaturation.

5. If one of the ſubſtances that combine, change from a ſolid to a liquid ſtate, it acquires the advantages poſſeſſed by ſolvents; and its affinity, which was diſguiſed by the ſolid ſtate, becomes active, ſo that the reſulting affinity may, on this account, be much more conſiderable than the elementary affinities appeared to be.

Thus, when ſulphur is diſſolved by pot-aſh, the ſulphuret which proceeds from it exerts a ſtrong action upon oxigen gas, as ſoon as it has been rendered liquid by the addition of water, or has attracted ſufficient humidity from the air; becauſe it has by that means loſt

* Fabroni has publiſhed ſome very intereſting obſervations on the mutual action of metals. (Journal de Phyſique, Brumaire, An. 8.)

its force of cohesion, as if it had been rendered liquid by the action of caloric, and because the pot-ash likewise exerts an action upon the oxigen, though much weaker than that of the sulphur, since it cannot alone overcome the elasticity of the gas. The action of the sulphur is really diminished by the whole of that attraction which it exerts upon the pot-ash, and upon the water which serves as a solvent to the sulphuret; but it gains much more by the liquidity which it acquires, than it loses by this saturation. Strictly speaking, all those substances of which the solidity is surmounted by a solvent, do act by virtue of a resulting force or affinity.

6. Circumstances contrary to the preceding produce an opposite effect; and when substances by combining, become solid, or more disposed to crystallise, this circumstance is added to the loss of force, arising from the saturation.

For example, pot-ash and nitric acid have both the property of dissolving in alcohol, and yet alcohol does not dissolve the nitrate of pot-ash; that is, the force of cohesion which belongs to that combination, and which, with water, produces its crystallisation, has modified the elementary affinities into the resulting affinity. What confirms this explanation is, that salts which are not crystallisable in water, because they oppose only a feeble cohesion, have, in general, the property of dissolving in alcohol; but, in such a manner, that they are capable of crystallising in that fluid, because the weaker action of the alcohol cannot overcome, except to a certain point, the force of cohesion, of which they are not entirely destitute. The limited solubility of nitrate of pot-ash in water, arises from the solubility of the pot-ash, as well as of the nitric acid by water being greater than in alcohol.

7. Bodies act in proportion to their quantity, which exists within the sphere of action. (Art. IV.)

Hence we deduce, that an action much stronger than that of the component parts may result from a combination, when the components, or one of the two, pass from the elastic to the liquid state; for they then carry into the sphere of activity a more considerable quantity, the action of which may considerably exceed the loss of force that is owing to the saturation.

Thus pot-ash cannot overcome the resistance which proceeds from the elasticity of the oxigen gas and the azote gas; but if these two be combined in order to form nitric acid in the liquid state, they act upon the pot-ash in a quantity much greater than when they could have applied in the elastic state; and the result of their action, though weakened by a commencement of saturation, is found to be much more considerable, than if the azote and oxigen had continued in the elastic state.

8. The affinity of a substance which enters into combination with a compound substance, concurs with the elementary affinities of the latter to maintain its composition against the action of foreign substances, in proportion to the degree of saturation which it produces. Thus iron easily carries off oxigen from azote, or rather, it shares it, or takes a portion; but as soon as the nitric acid is combined with pot-ash, iron can no longer separate its oxigen

oxigen at an ordinary temperature; but at a higher temperature the difference of dilation sufficiently destroys the resulting affinity of the pot-ash to cause the iron to combine with the oxigen.

In the oxigenated muriatic acid, the oxigen which has undergone but little saturation, and which consequently is feebly retained by the muriatic acid, passes easily into other combinations; but though it is found in a much greater proportion in the oxigenated muriate of pot-ash, it is carried off with much more difficulty by oxigenable substances.

The phosphate of lime is not decomposed by charcoal, even at a great degree of heat; but if it be in the state of acidulous phosphate, the portion of acid, which may be considered as in excess at the state of saturation, is capable of being decomposed by charcoal, because it is not prevented by a sufficiently large mass of base; and it is this part alone which affords phosphorus, when, in order to obtain this substance, we use phosphate of lime reduced to an acidulous phosphate by the sulphuric acid.

9. The contrary takes place when, instead of a saturating substance, which serves as a support to the resulting affinity, one is added that tends to form a combination in which one of the constituent parts must enter. For example, when sulphuric acid is added to a mixture of water and iron, this acid favours the decomposition of the water, because it tends to combine with the metal, and with a proportion of oxigen; a tendency concurring with that of the metal against the affinity which forms the combination of oxigen with hidrogen.

10. From the preceding remarks it may be concluded, that the properties of the resulting affinity of compound substances may be reduced, 1. to the advantages of liquidity, and under this point of view it is necessary to apply to it the theory of solvents (Art. IX.); 2. to the disposition to solidity, which produces contrary effects, which must be explained by the force of cohesion (Art. V.); 3. lastly to the concentration of elastic substances; this circumstance demands particular consideration, but it may be found combined with one of the two preceding.

The observations presented in No. 7 and No. 8 prove, that in those compounds in which elastic substances are concentrated, there are established by a change of constitution, affinities which may be considered as new; an additional force has accrued, to which we may apply the inverse of what has been stated relative to the effects of elasticity (Art. VI.)

The distinctive character of the complex affinities, of which I have treated (Art. XII.) compared with those which result from the composition of the substances of which I now speak, is, that in the first but very little change obtains in the constitution of the component parts; so that when the force of cohesion or elasticity does not intervene, we may consider them in the same manner as they have been considered in Article XII, No. 1. whereas a new force is established in compounds in which elastic substances are condensed; a power which may be considered analogous to that of the cohesion which obtains on the mixture of different substances that determine the combinations that are formed, or which require to be overcome by the forces opposed to it.

11. Caloric, by augmenting elasticity, destroys the affinity resulting from substances, of which the constituent parts have an unequal dilation, conformable to what has been stated in Art. VII.

12. Observation farther shews us, that when the resulting affinity is not sufficient to prevent the decomposition, it sometimes renders it very slow and long. It is to this slowness of action, to these progressive changes of constitution, to the different degrees of saturation which take place, that most of the phenomena observable in vegetation, fermentation, the animal economy, and in general among all bodies which contain condensed elastic substances, must be ascribed*. This subject will require still farther explanation.

13. The resulting affinity ought always to be considered as a single force, while the substances from which it is derived remain in combination: but it is necessary to consider the elements of which it is composed when a separation takes place; the latter is then executed conformably to what has been explained in the division of substances, in proportion to the opposite powers which act upon them.

14. It frequently happens that a substance acts partly by a resulting affinity, and partly by its elementary affinities. When a metal is dissolved by the nitric acid, one part of the acid exerts a resulting affinity, and another acts by its elementary affinities, so that the oxigen of the latter part is divided between the metal and the azote, and the oxide which is formed is dissolved in the undecomposed acid.

15. It is evident, from what has just been explained relative to resulting affinity, that a false idea may be adopted of the properties of a body, when we confine ourselves, as is too often done, to the determination of its constituent parts, without paying attention to the other conditions of its constitution, if, among those constituent parts there may be some which have undergone a considerable alteration in their state. A quantity of oxigen gas does not possess the same chemical power when it is in the elastic state, as when it exerts a resulting force in its combination with azote, hydrogen, carbon, sulphur, or a metal.

For example, the oxigen does not exert the same action, and has not the same resulting affinity, in the sulphuric and the sulphureous acid: though in the sulphuric acid a smaller proportion of sulphur is combined, yet it adheres much more strongly than in the sulphureous acid, and being more condensed, it exerts a much more powerful chemical action†. We ought not to confound the oxigen gas which is held in solution by water, with the oxigen which, by its combination with hidrogen, forms this liquid: the difference caused between them by the state of condensation, produces two substances very different in their chemical action.

* I have often had recourse to this change of constitution in the chemical explanations which I have had occasion to give, and particularly in the lectures of the National School, where I defined the resulting affinity by the name of *collective affinity*, and distinguished it from *elementary affinities*.

† I have observed the effects of condensation in a memoir on the sulphureous acid (Annales de Chimie, 1789).

It

It is therefore neceſſary either to conſider all the conſtitution of a body, in order to explain its chemical properties, or to content ourſelves with eſtabliſhing thoſe properties by experiment; for we muſt pay attention to all the circumſtances of chemical action, to explain the reſults of that action, or we muſt confine ourſelves to their eſtabliſhment or confirmation.

ARTICLE XV.

Recapitulation.

1. It has often been remarked that the action of a ſubſtance is diminiſhed, in proportion as it approaches to a ſtate of ſaturation: and this diminution of power was made uſeful in the explanation of ſeveral chemical phenomena. It was affirmed that a metal could only take a definite portion of oxigen from the nitric acid, becauſe when the portion of oxigen in the acid is diminiſhed, the remainder is too ſtrongly combined with the azote. The property of carrying off only a portion of oxigen from certain metallic oxides was attributed to hidrogen; it was admitted that when a ſubſtance attracts humidity from the air, its diſſolving force comes to an equilibrium; ſo that according to the degrees of deſication of the air, it may either carry off or give out water to it. It was known that the reſiſtance found in expelling a ſubſtance from the laſt portions of a combination, either by the action of an affinity, or by that of heat, is much greater than at the commencement of the decompoſition, and ſometimes ſuch, that its entire decompoſition cannot be effected. Thus it was aſcertained that oxigen could be but partially diſengaged from the oxide of mangameſe by the action of heat.

The combinations which are formed when forces are oppoſed, does not therefore depend upon the affinities alone, but upon the proportions of the ſubſtances which act. I have therefore only applied to all the phenomena of chemiſtry, what has been unavoidably admitted in ſeveral of them from obſervation. I have deduced the immediate conſequences.

2. Theſe conſequences are, that ſubſtances act in proportion to their affinity, and their quantity exiſting within the ſphere of activity; that the latter may compenſate the force of affinity, and that the chemical action of each power is proportionate to the ſaturations it produces. I have defined by the word *chemical maſs*, or *maſs*, the quantities determined by a like degree of ſaturation, and conſequently relative to the capacity of ſaturation: when two ſubſtances are in competition in order to combine with a third, each of them obtains a degree of ſaturation proportionate to its maſs. The ſubject of the combination alſo divides its action in proportion to the maſſes, and by varying the latter the reſults will alſo be varied.

3. I have conſidered all the forces which, by their concurrence or oppoſition to the mutual affinity of the ſubſtances brought in action according to the preceding principle, may have an influence upon chemical combinations and phenomena. They may be reduced to the following heads: the action of ſolvents, or the affinity which they exert according

to their proportion; the force of cohesion, which is the effect of the mutual affinity of the parts of a substance or combination; the elasticity, whether natural or produced by heat, which ought to be considered as an affinity of caloric; the efflorescence, the cause of which may be attributed to an affinity which is not yet determined, and which acts only under very rare circumstances; gravity likewise exerts its influence, particularly when it produces the compression of elastic fluids; but it may always, without inconvenience, be confounded with the force of cohesion.

4. I have endeavoured to ascertain whether it were possible to determine the relative affinity of two substances for a third; I have observed that for this purpose it would be necessary to discover in what proportions this third would be divided with a given quantity of each of the two first, or rather would divide its action; I have pointed out the insurmountable obstacles that would be met with in the means that must necessarily be employed to prove this division of action, and the changes of constitution which would attend it.

5. As the tables of affinity have all been constructed upon the supposition that substances possess different degrees of affinity, which produce the decompositions and combinations which are formed, independently of the proportions and other conditions which contribute to the results, these tables are calculated only to give a false idea of the degrees of chemical action of the substances arranged in them.

6. The denomination of *elective affinity* is, in itself, erroneous, since it supposes the union of one entire substance with another, in preference to a third, while there is only a division of action, subjected to other chemical conditions.

7. The action of two, three, or a greater number of substances, is subject to the same laws, and the result depends on their affinity, their proportion, or the degree of saturation in which they are found, and the concurrence or opposition of the forces they exert.

In every case of liquidity a mutual saturation takes place, and the result is a single combination, in which all the forces are counterbalanced, while there is neither precipitation nor disengagement of elastic matter; but as the action is divided when there is an opposition of forces and a difference of saturation, some substances are retained in the new combination less strongly than before the mixture; they may consequently yield to the powers of cohesion, elasticity, or other affinities which they might otherwise have resisted.

8. The force of cohesion which has hitherto been considered merely as an obstacle to solution, limits not only the quantities of substances which may be brought into action in a liquid, and consequently modifies the conditions of the saturation which follows; but it is the power which causes the precipitations and crystallisations that take place and determines the proportions of such combinations as are made by quitting the liquid: it is this force which sometimes even produces the separation of a substance, without its forming any combination with another substance, as we have remarked in some metallic precipitations. I have distinguished insolubility from the force of cohesion, because the one is relative only to the action of the solvent, and the other is the effect of the mutual affinity of the parts of a substance or combination, considered absolutely.

3 Elasticity

Elaſticity acts by producing effects oppoſite to thoſe of coheſion, and which conſiſt either in withdrawing ſome ſubſtances from the action of others in a liquid, or in diminiſhing the proportion which exiſts within the ſphere of activity; but when all the ſubſtances are in the elaſtic ſtate, their action is ſubjected to the ſame conditions.

If tables were formed which would repreſent to diſpoſition, to inſolubility, or volatility, in the different combinations, they would ſerve to explain a great number of combinations which take their origin from the mixture of different ſubſtances, and from the influence of heat.

9. Caloric acts upon bodies like the other ſolvents, when it is not in a ſtate of *radiant caloric*, becauſe in this caſe it is not in combination.

It is neceſſary that it ſhould exceed the greateſt part of the force of coheſion, in order to render a body liquid, and other affinities may concur with it to produce this effect, in the ſame manner as itſelf concurs with the action of other ſolvents.

It is not diſtributed among bodies in proportion to their ponderable quantity, or their bulk, when it produces the degrees of temperature indicated by rhe thermometer, in the ſame manner as an acid does not take up an equal quantity of the different alkalis, to attain the ſame degree of ſaturation, and the tables of ſpecific caloric are ſimilar to thoſe of ſpecific acidity or alkalinity, which might be conſtructed: the former determine the capacity of caloric, the latter would determine the capacity of ſaturation *.

One difference, however, would exiſt between theſe tables is, that thoſe of acidity or alkalinity would repreſent the whole ſaturation to a certain aſſumed point, becauſe pure acids and alkalis might be employed: but the ſpecific calorics could only be determined from an unknown point of ſaturation to another point, becauſe the bodies ſubmitted to experiment are already combined with a quantity of caloric. The reſults which may be obtained between two degrees of the ſcale of the thermometer, have no known connection with the total quantities. To attempt to form a concluſion of one from the other, is the ſame as to pretend to determine the comparative ſolubility of the muriate of ſoda and the nitrate of pot-aſh in water, by experiments made only at or towards the point of ebullition or of congelation. In the former caſe we ſhould ſay that three parts of water would be required for the ſolution of one part of muriate of ſoda, and only half a part would be neceſſary to diſſolve one part of the nitrate of pot-aſh; in the latter that much leſs water would be required to diſſolve the muriate of ſoda than to diſſolve the nitrate of pot-aſh †.

* Several chemiſts have buſied themſelves in determining the conſtituent parts of chemical combinations: no perſon has carried this point to ſuch an extent, and with ſo much ſucceſs, as the celebrated Kirwan; though the methods he employed are ſubject to ſome diſcuſſion.

The ſame chemiſt has eſtabliſhed that the affinities are proportionate to the quantities producing the ſaturation; but he conſidered them as forces independant of the proportions and other conditions which modify them.

† This conſideration alone, that ſpecific caloric has no known relation to the quantity of caloric combined in a body, ſhews that the experiments by which Rumford has lately pretended to prove that caloric is not a conſtituent part of bodies, cannot lead to ſuch a concluſion.

On taking the ſolid ſtate, the force of coheſion in a body obliges a part of the caloric to ſeparate; in the ſame manner as when a ſalt cryſtalliſes, it abandons part of the ſolvent, or even a part of the acid, or of the alkali, with which it might be combined.

10. It might be affirmed, that the affinities would be really repreſented by the tables of capacity; ſince they would afford the meaſure of the action of ſuch ſubſtance upon another, when a common term of ſaturation was found, ſuch as neutraliſation for the acids and the alkalis, and thermometric temperature for caloric; but nothing could poſſibly be concluded for chemical action at another term of ſaturation, and more particularly for another conſtitution, and for all the circumſtances in which the forces of elaſticity and coheſion might not be introduced.

11. After having conſidered all the affinities which may jointly produce chemical action, I examined how, in the compounds, they may reſult from their conſtituent parts, in order to acquire a conception how the varied powers which produce all the chemical phenomena, can be derived from one ſingle property of ſimple bodies.

The obſervations preſented on this ſubject have ſhewn, that what principally diſtinguiſhes compound ſubſtances, whoſe action is conſidered as ſimple is, the condenſation of the conſtituent parts, on which a new affinity depends, an affinity very different from that which the ſame parts poſſeſs in the elaſtic ſtate: the elementary affinities are modified by the ſtate of ſaturation, by the force of coheſion, or by the variations of elaſticity: the reſulting affinity may undergo, by combination, a new degree of ſaturation which tends to ſupport the compoſition, or may be weakened by other tendencies to combination with one of the conſtituent ſubſtances.

12. All the conſiderations which I have preſented reſpecting the modifications of chemical action, do not prevent us from uſing the term affinity to denote the whole chemical power of a body exerted in a given ſituation, either by its preſent conſtitution, its proportion, or even by the concurrence of other affinities; but we muſt avoid conſidering this power as a conſtant force which produces compoſitions and decompoſitions. By ſo doing we ſhould erroneouſly infer from its preſent effects in any inſtance, what it would be under other conditions which may indeed give it a very different degree of force; by ſo doing we ſhould neglect all the modifications it may experience from its initial action to that in which it obtains its equilibrium.

13. In this eſſay I have pointed out an uniform proceeding for the cultivation of chemical ſcience. I have conſidered all the forces which unite in the combinations and phenomena on which it is employed; I have endeavoured to determine the influence of each in different circumſtances. If the reader ſhould regret the loſs of all expectation of claſſing the chemical power of bodies, independently of the conditions which modify it, it will readily be granted, that the tables of affinity contained ſimple notes of facts nearly ſterile, and that they deprived us of the advantage of employing in our deductions the knowledge of the moſt fruitful properties, by means of which a great number of the reſults of chemical action may be anticipated; and the facts may be interpreted and directed without having recourſe to ſuppoſitions, or requiring any particular principles to explain anomalies.

V.

A Memoir on Mortars, Water Lime, Cements, and Pouzzolana. By CIT. GUYTON.

WE find in many publications accounts of proceſſes for the preparations of mortars and cements; enquiries into the compoſition of thoſe employed by the ancient in edifices ſtill reſpited by time; experiments to determine the condition of their ſolidity in the different circumſtances; obſervations on the materials proper to each of the ſeveral uſes; and deſcriptions of the characters. It has appeared to me that by collecting and comparing the facts and inferences, we might obtain a ſmall number of truths no longer ſubject to diſcuſſion, which cannot be rendered too familiar to mankind. This is the object of the preſent memoir, in which I ſhall alſo ſtate ſeveral experiments hitherto unpubliſhed.

Loriot Mortar.

In 1775, the late M. Loriot directed the mixture of one-fourth part of powdered lime in a good mortar, made as uſual of lime and ſand, as the eſſential conditions for obtaining buildings as ſolid as thoſe of the ancient Romans. See his Memoir, Journal de Phyſique, III. 231.

Having obſerved that the pulverization of the lime is a painful and even dangerous operation for the workmen, who, notwithſtanding every precaution, were ſubject to bleeding at the noſe. I propoſed to have the lime covered till it became ſpontaneouſly ſlaked into powder, and then to calcine it again in a ſmall furnace, and uſe it hot.

This proceſs is explained at full length in a memoir printed in the fourth volume of the Journal de Phyſique, page 481. It has been frequently uſed in the large way, and always with the moſt perfect ſucceſs; which is not to be wondered at, when we conſider that two very deſirable conditions are thus obtained, namely, that the lime is freſh and in very fine powder.

The deſcription of the oven, with plans, ſections, and profiles, is to be found in an inſtruction printed in 1775, by order of government, and in the Journal de Phyſique for October of the ſame year, Vol. VI. page 311. The dimenſions are ſufficient to afford an uninterrupted ſupply to the labour of a number of workmen, and the proportions of the compoſition of the mortar, which were thought to be the beſt, were:

Fine ſiliceous ſand - - - - -	0. 3
Cement of well-baked bricks - - -	0. 3
Slaked lime - - - - - -	0. 2
Lime ſpontaneouſly ſlaked, and again calcined -	0. 2
	10.

Mortar

Mortar of Lafaye.

In 1777, Lafaye publiſhed his enquiries into the preparation which the Romans gave to their lime, and the compoſition of their mortars. The practical part is extracted, with much detail, in the Journal de Phyſique, IX. Part I. p. 437.

The eſſential article is the method of ſlaking the lime without liquefying it, in order to have a powder, and ſtill ſufficiently cauſtic to afford a very ſtrong lime, which ſhall grow hard with age.

For this purpoſe good freſh lime, made from hard ſtone, is taken and broken into pieces about the ſize of an egg; theſe are thrown into an open baſket, which is plunged into water, and kept there till the ſurface begins to boil; at which period it is taken out, and ſuffered to drain, when the lime is thrown into caſks, where it heats, and falls into powder.

This lime, ſo prepared, is taken as an ingredient in various kinds of mortar, according to the object. It keeps very well when the caſks are covered with ſtraw.

I have uſed this lime in the conſtruction of a ſmall aqueduct of more than fifty metres in length, intended to convey water to an artificial nitre bed near Dijon, about eighteen years ago. The mortar made of three parts of ſand, and three of cuttings of hard lime ſtone, became very firm and ſolid in a ſhort time, though it was under ground.

Concerning Poor Lime, or Water Lime (Chaux Maigre).

In ſome countries they have a kind of lime called poor lime, becauſe in equal quantities it does not give a mortar as fat as the others; but this lime has a property which renders it very valuable, namely, that of becoming ſolid and hard under water.

The cauſe of this property was unknown till Bergman obſerved that it is owing to a oxide of manganeſe, which the lime ſtone of Lena contains in the proportion 0.02, as do likewiſe other ſtones that form a mortar capable of hardening under water. Opuſc. II. Eſſay XIX. §. 10.

In 1783, I publiſhed, in the ſecond ſemeſtre of the new memoirs of the academy of Dijon, page 90, experiments on ſix ſpecies of this kind of lime ſtone, among which was the lime ſtone of Lena, which had been ſent me by Bergman. Two among them only aſſumed by calcination the brown colour which characteriſes manganeſe, and underwent comparative eſſays with the lime ſtone of Lena.

Independant of the poor lime ſtone indicated in that memoir, namely, thoſe of Lena, Brion, in the department of Saone et Loire, at the diſtance of five kilometres from Autun, and of Morex, near Geneva, I am at preſent acquainted with two others, viz. 1. that employed at Metz, in the works under water, of which the analyſis was made in the courſe

courſe of the polytechnic ſchool in the year 6;* and, 2. that which Smeaton, conſtructor of Eddyſtone light-houſe, has deſcribed in his account, which was obtained from Aberſhaw, in the county of Glamorgan. Bibl. Britanique, September, 1796, page 102.

I have no doubt but that ſtones of the ſame quality may be found in many other places, and in the territory of the French republic †, that is to ſay, containing mangaueſe, and naturally mixed with a quantity rather conſiderable of ſilex and alumine; for it is evident that this laſt condition contributes to afford a poor lime ſtone, or mortar, nearly prepared by nature, and unfit to be mixed with any conſiderable quantity of ſand, or cement, or other dry matter.

Artificial Water Lime (*Chaux Maigre*).

I have long ago propoſed to uſe, in caſes when the natural lime ſtone is not to be had, a mixture of four parts of grey clay, and ſix of the black oxide of manganeſe, with ninety parts of good lime ſtone reduced to a powder.

This mixture, after having been well calcined and cooled, is worked to the conſiſtence of ſoft paſte, with ſixty parts of ſilex.

In the laboratory of the polytechnic ſchool is preſerved a ball of this mortar thus prepared, according to the above proportions, at the ſitting of the 29 Pluvioſe, in the year 5, which was immediately thrown into water, where it hardened, and acquired the ſpecific gravity of 2.231.

A very good water lime may be procured ſtill more cheaply by mixing, with common quick lime, a certain quantity of the white iron ore, which is found plentifully, and is in great part compoſed of carbonate of lime with manganeſe, more eſpecially thoſe which are poor in iron.

It is poſſible, as De Sauſſure obſerves in his travels in the Alps, that there may be ſtones, which though they contain no manganeſe, may from ſome other particulars of their compoſition, nevertheleſs afford lime capable of hardening under water, ſuch as the aſhes Cendree of Tournay, mortar made with the traſs of the Hollanders, &c. But this in no reſpect weakens the reſults of obſervations and experiments on the efficacy of the oxide of manganeſe; as for inſtance that of common mortar, which without this addition would immediately become diffuſed in water, but hardens very ſpeedily when a certain doſe of that oxide enters into its compoſition. Mr. Smeaton here affords us an important remark, namely, that balls made of lime and ſand well worked together, and then put into water, were deſtroyed, whatever were their proportions (Loco cit.)

* In this analyſis it was found to contain in the hundred carbonic acid 39.0; lime 44.5; ſilex 5.25; alumine 1.25; manganeſe 3.5; oxide iron 3.2; water 2.25; loſs 1.05.

† Citizen Lomet found a quarry at Saintrails, in the department of Lot-et-garon, which has been ſucceſsfully employed in building under water.

Concerning

Concerning Beton, or Water Cement.

Every compofition of mortar intended to hold under water, and capable of becoming almoft inftantly folid, is called in French Beton.

This water cement may be made in feveral manners: 1. the mortar Loriot, if well made, holds very well under water. I have ufed it in the walls of a terrace through which there was not the fmalleft filtration. I have a mafs of this mortar hollowed out into a bafon, which when filled with water, and placed on a fheet of paper, does not fhow the flighteft trace of moifture.

The mortar Lafaye hardens lefs fpeedily, but when once it has become folid, it holds water very well without being foftened.

3. The mortar fpoken of by Smeaton, compofed of equal parts of lime and trafs, or the volcanic afhes of the Hollanders, is a true water cement, as it does not yield at all to that fluid.

4. The compofitions of mortar with Pouzzolana may alfo be ufed under water, and when once indurated, they furpafs all others for the hardnefs and refiftance to the motions of the waters.

The moft common water cement is made with the poor lime ftone, or water lime. It is formed into prifms in hard pafte, which are let down into the water, and form a wall, or otherwife this material is poured between two walls, at the diftance of 21 or 22 metres afunder, in the manner of coffer work, for the purpofe of forming a fuitable defence or inclofure for waters in parts of canals above the furface of the ground.

Methods of fupplying the Want of Pouzzolana.

Pouzzolana is a volcanic fand which is brought from the environs of Naples. Though the fuperiority of mortar formed with this material is well known and determined, yet it is often neceffarily difpenfed with on account of the expence of carriage, which fo greatly enhances the price.

But as we are well acquainted at prefent with a number of extinct volcanos in France, we may find an abundance of materials to fupply the place of the Pouzzolana. The experiment was made in 1787 by Citizen De Ceffart, engineer, charged with the conftruction of the cones of Cherbourg. In the month of Auguft of that year I fent him about 13 kilograms of the extinct volcano of Drevin in the department of Saone-et-Loire.

The bafaltes was firft heated in a fmall reverberatory furnace, then thrown red-hot into water, and ftamped and fifted, fo that the largeft grain did not exceed the fize of a pea. The following are the refults of experiments on mafonry conftructed with this cement of bafaltes, compared with another cement from the Italian Pouzzolana, according to the letter of Citizen Ceffart, dated Feb. 12, 1788.

"In

"In order to afcertain perfectly the tenacity of this cement, I caufed moulds to be made fix inches in length, fix in width, and four in thicknefs, forming fmall maffes of 144 cubic inches.

"Compofition of the cement, 180 cubic inches of bafaltes, 73 of lime flaked (fufeé à la romaine) 93 inches of fpring water, and 288 ftones of granite of the fize of a nut, the whole forming a cube of 534 inches.

"Two moulds of a 144 inches each were filled with this compofition. One of thefe was immerfed in the fea for 76 days, the other remained in the air for the fame time.

"When the tenacity of this mafonry was to be tried, the extremities of thefe maffes were placed on the edge of a piece of iron: each of them was loaded with a kind of wooden trough, very narrow at bottom, placed acrofs the middle of the mafs, and gradually loaded with balls of lead.

"The fame operation was formed with two fimilar bricks of Italian Pouzzolana, having the fame proportions, and having remained likewife 76 days in the fea. It supported 123 pounds before it broke, whereas the piece which remained 76 days in the air, broke under a charge of 71 pounds."

All thefe proportions, as the writer obferves, are infinitely too fmall to eftablifh an exact ratio, but it may neverthelefs be concluded, that the Pouzzolana bafaltes of Burgundy is nearly as good as that of Italy, which comes to us by Toulon.

Citizen Ceffart in the fame letter, demanded fix thoufand weight of this Pouzzolana to repeat his experiments on a larger fcale; to which it appeared, that the difficulties and expence of tranfportation were at that time an obftacle. But this would no longer be any impediment; as the canal of the Saone and the Loire is in full navigation, and the extinguifhed volcano of Drevin is no more than one myriameter diftant from it.

Another material ftill more common than bafaltes, and which may fometimes be a fubftitute for pouzzolana, is the afhes of pit coal, provided it be a heavy cakeing coal, leaving behind it rather a fcoria than a cinder.

I likewife fent this article to Cherbourg in 1787, under the name of light pouzzolana, to diftinguifh it from that which was made with bafaltes. It was fubmitted to the fame comparative experiments, and afforded the following comparative refults, as is ftated in the letter of Citizen Ceffart.

"The bricks which had remained 76 days in the air, fupported charges of 115 and 137 pounds before they broke.

"The fame blocks immerfed in the fea likewife for 76 days, broke with 48 and 30 pounds, confequently they had loft their confiftence in the water of the fea."

We obferve, that mortar made with this afhes is evidently fuperior to that formed with the pouzzolana of Italy, as its tenacity is almoft double. It cannot indeed be employed under water like the cement of water lime, but it is probable that it would fucceed very well in works which are conftructed dry, and are not to receive water until they have acquired folidity.

 With

With this view I pointed out this preparation for the maſonry of the inner faces of the canal of Saone-et-Loire, which were preciſely in this condition, and near which there were conſiderable maſſes of this matter afforded by the fires of a glaſs-houſe.

ADDITION.

Citizen Monge, who viſited the ruins of Ceſarea, of which the hiſtorian Joſephus has given ſo magnificent a deſcription (Cæſarea Stratonis) at preſent abſolutely without inhabitants, aſſures me, that he could diſtinguiſh in its ruins, by the taſte of the architecture and the ſolidity of the mortar, the different ages of the firſt conſtruction, and the works ſucceſſively added after the invaſion of the Arabs, and during the Cruſades.

" He obſerved on the ſide of the gate at preſent in ruins, the remains of a temple conſecrated to Auguſtus. Endeavours had been made to ſupport the walls in front by counterforts ſtill in being, which in ſome places exhibited a concave mould or impreſſion of ornaments of an exquiſite taſte formed of mortar, which had applied itſelf againſt reliefs which are now no longer in being.

The ſtone of the original ſtructure was corroded by the weather to a great depth, and the mortar projected out beyond it.

He attempted in vain to break off a piece. It was of a very fine and equal texture, apparently compoſed of fine ſand and very little lime well blended together.

I thought that this obſervation might be received with pleaſure at the end of the preſent memoir.

VI.

On the Combinations of the Metals with Sulphur, by Cit. Vauquelin *.

WE may divide the combinations of the metals with ſulphur into three orders, namely, firſt metals and ſulphur, which are combinations properly denominated metallic ſulphurets, or ſulphurated metals. Nature and art preſent many ſamples of this kind. 2. Metallic oxides and ſulphur; theſe ought to be called ſulphurated metallic oxides, or oxided metallic ſulphurets. They are alſo very common in nature. 3. Metallic oxides with ſulphur and hydrogen. Theſe triple combinations are called metallic hydro-ſulphurated oxides.

If the ſubſtance which predominates in theſe different combinations be taken as the baſe of the nomenclature, we ſhould reverſe the order of the common denominations; for in the ſimple ſulphurets the maſs of metal always predominates, and ſo likewiſe in the ſulphurated oxides; but in the hydro-ſulphurets at firſt the metals, then the ſulphur, and afterwards the hydrogen predominate.

* Annales de Chymie, XXXVII. 57.

So

So that inftead of metallic fulphurets, oxided metallic fulphurets and metallic hydro-fulphurets, we ought to fay fulphurated metals, fulphurated metallic oxides, and fulphuro-hydrogenated metallic oxides.

But cuftom has predominated in chemiftry, either becaufe the preference has been dictated by the facility of pronunciation, or becaufe the terms themfelves are lefs harfh. This however is of no great importance, as it is fufficient that the elements of bodies fhould be expreffed by their names, and the value of thefe names fhould be precifely underftood.

Concerning the action of the acids on the metallic fulphurets.

Sulphuric acid.

Whenever the fum of the affinities of oxigen for a metallic fubftance, and of the metallic oxide formed by the fulphuric acid, exceed thofe of the metal for fulphur, and of oxigen for the fulphureous acid, there is conftantly a decompofition of the metallic fulphurets; and the refult is a metallic fulphur and fulphurous acid. All thefe conditions are not conftantly neceffary, but every one of them concurs in accomplifhing the new combinations which are formed, and accelerating the procefs. There are neverthelefs cafes, in which for want of one of thefe conditions no action takes place between the bodies.

The fulphuric acid for example, decompofes the natural fulphurated oxide of iron, or martial pyrites very well, but the muriatic acid cannot decompofe it, becaufe the fulphur in this cafe is not attracted by any body, whereas in the other cafe it is attracted by the fulphuric acid, which it converts into fulphuric acid; for though I am affured by many facts, that the iron in martial pyrites is oxided nearly to the point at which it is eafily foluble in acids, neverthelefs thefe compounds cannot be decompofed without the formation of a great quantity of fulphurous acid.

This opinion is confirmed by the conditions required for the fuccefs of the operation. In fact, the complete decompofition of pyrites is not effected but by means of heat, and a large quantity of concentrated fulphuric acid; for it is evident, that in this cafe the acid is required not only to faturate the oxide of iron, but alfo the fulphur. I might alfo urge in proof of the fame opinion, the greater affinity of the muriatic acid for the oxide of iron which it cannot neverthelefs feparate from the fulphur in thofe circumftances. It may feem aftonifhing, that the nitric acid, which has a lefs affinity with iron, fhould neverthelefs decompofe the martial pyrites with fo much eafe; but we muft not deceive ourfelves in this refpect, by fuppofing that the acid takes the oxide of iron from the fulphur by a ftronger affinity, for it is on the contrary, by giving a new quantity of oxigen to that fubftance, that its affinity for the fulphur is remarkably diminifhed. The fulphur does not unite in fact with iron oxided to the maximum, and if by any peculiar proceffes thefe fubftances become combined, a portion of oxigen is then feparated.

Though the muriatic acid cannot decompofe the fulphurated oxide of iron, it decompofes on the contrary with the greateft facility the fulphuret of iron, in which the metal is not oxigenated. But in this cafe three forces act at once, namely, that of the iron on the

the oxigen of the water, that of the ſulphur on the hidrogen of the ſame ſubſtance, and that of the metallic oxide formed by the muriatic acid. Under theſe circumſtances ſulphurated hidrogenous gas is conſtantly diſengaged.

The ſulphuric acid which does not decompoſe the ſulphurated oxide of iron, unleſs it be concentrated and boiled, decompoſes the ſulphuret of iron in the cold, and the more eaſily when diluted with a certain quantity of water. The reaſon of this is evident, and need not be explained.

Though a great quantity of water is inimical to the action of the acids upon metallic ſulphurets in general, it is however leſs ſo with regard to iron than other metals, becauſe ſulphurated hidrogen does not decompoſe the ſolutions of iron in acids.

Concerning the Sulphuret of Lead.

In the natural ſulphuret of lead or galæna the lead exiſts in the metallic ſtate, for which reaſon when this mineral ſubſtance is put in contact with muriatic acid, ſulphurated hidrogen is formed even in the cold. It appears even when ſulphur is united to the oxide of lead. That metal loſes its oxigen to unite to the ſulphur for the combination by this means obtained, is perfectly ſimilar to natural galena. It is eaſy to conceive how the reduction of the lead is obtained in this proceſs: a portion of the ſulphur combines with the oxigen, and forms ſulphuric acid, which is driven off by the heat, while another portion of the ſulphur unites to the metallic lead. The reduction of the lead, and the formation of galena take place nearly in the ſame manner, when a ſolution of this metal in an acid is mixed with a ſolution of ſulphurated hidrogen, or any hydro-ſulphuret whatever. There is however this difference, that in the laſt caſe it is hidrogen, which takes oxigen from the lead, and forms water at the ſame time that the ſulphur and the metal, which meet in a very extreme ſtate of diviſion, combine without heat in the midſt of the fluid. There is alſo another difference, that the galena formed by this proceſs contains a little more ſulphur than the natural compound, as is proved by the ſmall quantity of ſulphur which is ſeparated from that ſubſtance by a very gentle heat, and then leaves a compound in a ſtate abſolutely ſimilar to that of common galena.

From theſe facts it reſults that the muriatic acid, and probably all the acids which act upon the oxide of lead by their maſs and their concentration, do loſe their lead by the action of the ſulphurated hydrogen, as ſoon as theſe two forces, or even one of them is weakened; but whatever may be the quantity of acid united to lead, or any other metal, the decompoſition of the ſalt will take place, if any hydro-ſulphuret be employed in ſufficient quantity.

We cannot be ſurprized at the diminution of the affinities of the acids for the metallic oxides by water, nor their more powerful action on theſe ſubſtances by their maſſes. Antimony, mercury, biſmuth, and lead itſelf, afford ſtriking examples of this.

We know in fact, that by mixing a great quantity of water with their concentrated ſolutions, very conſiderable precipitates are formed, which have been erroneouſly taken for

pure

pure metallic oxides, but which are really ſalts, containing a portion of acid too ſtrongly adherent to be ſeparated but by means of an alkali.

We may therefore conſider the combinations formed by theſe metals, and probably by many others likewiſe with acids as neutral ſalts, diſſolved in a ſuperabundance of acid, which is taken from them by water, when the ſalts are precipitated in a truly neutral and inſoluble ſtate.

From the manner in which the muriatic acid acts upon the metallic ſulphurets, it follows, that the more oxigen is abſorbed by the metals, in order to unite with this acid, the leſs of ſulphur will remain after the ſolution, and the contrary; but if theſe metals abſorb but little oxigen, if they contain much ſulphur, and if alſo their affinity for oxigen be not conſiderable, the whole of the metal will not be ſeparated from the ſulphur by the muriatic acid, becauſe in proportion as this combuſtible body became more abundant, it oppoſes the ſolution of the metal by virtue of its maſs; this is the reaſon why certain metallic ſulphurets, which are not attacked by the muriatic acid, are decompoſed by it, when a portion of their ſulphur has been ſeparated in diſtillation.

The mode of action of the nitric acid upon the metallic ſulphurets is very different from this, for though the ſulphur is not in this caſe attracted by any body, it is nevertheleſs completely ſeparated from the metal, and thrown down in all its purity, if the experiment be properly conducted: but if the combination of the nitric acid with the metal be not aided by hidrogen, that takes place with the muriatic acid, it gives out to the metal a portion of oxigen much more readily than water; and it is in conſequence of this affinity of the oxigen for the metal, and of the oxide which is formed for the undecompoſed acid, which is effected by the ſolution. Thus every time that metallic ſulphures are decompoſed by the nitric acid, there is a conſtant formation of nitrous gas, and never of ſulphurated hidrogen.

It appears that in general ſulphur has a greater affinity for the metals than for their oxides, and that this affinity diminiſhes in proportion to the quantity of oxigen they abſorb: they are ſome which do not even combine with ſulphur, except in the complete metallic ſtate; thus for inſtance, on triturating ſome oxide of lead with ſulphur, a portion of the latter deflagrates, and the other combines with the lead, which is brought to the metallic ſtate.

Hence it appears that zinc does not combine with ſulphur when it is completely metallic, at leaſt nature has not hitherto afforded us ſufficient examples, and art has not ſucceeded in effecting the combination of theſe two ſubſtances. What is known in mineralogy under the name of *blende*, or ſulphurated zinc, has been improperly conſidered by ſome mineralogiſts as a combinaion of metallic zinc with ſulphur. It is, on the contrary, oxide of ſulphurated zinc, which very frequently contains hidrogen; for, the *blendes* almoſt always diſſolve in the acids, and produce ſulphurated hidrogen gas. Thus a tranſparent blend which acts in this manner with the acids, ought to be conſidered as a real oxide of hidro-ſulphurated zinc, or rather ſulphureo-hidrogen. There may be oxides of zinc ſimply

ſimply ſulphurated, and theſe will then be two ſpecies of minerals very different in their nature; the one diſſolves in the ſulphuric and muriatic acids without effervefcence, and the other, on the contrary, affords a greater or leſs quantity of ſulphurated hidrogen gas.

Mercury, in a metallic ſtate, does not very intimately combine with ſulphur; ethiops mineral appears to be rather a mixture than a real combination; but when this oxided metal is combined with ſulphur, as in cinnabar, for example, it adheres ſo ſtrongly, that no ſubſtance is capable of directly decompoſing it; and it is only by altering one or the other of theſe principles that their ſeparation can be effected, or rather it is neceſſary to burn the ſulphur or diſoxide the mercury; cinnabar owes its fine red colour only to the great quantity of oxigen which the mercury contains.

VII.

On the Precipitate formed by mixing Muriate of Lime and Muriate of Magneſia. By W. Lambe, M. A.

To Mr. NICHOLSON.

SIR,

HAVING diſcovered, what I conceive to be, the true explanation of the appearance of a precipitate by mixing the muriate of lime and magneſia, I take leave to communicate it to you. All the accounts I have met with on this ſubject are erroneous; and that offered by the celebrated Berthollet is as much ſo as any (Journal, IV. 67). To refute it, I need only mention a ſingle fact. Let the carbonate of magneſia, before diſſolving it in the muriatic acid, be thoroughly deprived of its carbonic acid by heat, and then there will be no precipitation by mixing the two ſalts. To the carbonic acid, then, is this appearance to be attributed, of which the carbonate of magneſia cannot be wholly deprived by the addition of ſo much muriatic acid as will produce an apparent ſaturation.

Diſſolve ſome common magneſia in a ſolution of carbonic acid in diſtilled water. To the clear ſolution add a little muriate of lime, and a precipitate is immediately formed, entirely ſimilar, I believe, to that in queſtion. If the liquor be examined after ſome days, carbonate of lime is found diſſolved in it, inſtead of carbonate of magneſia. It ſeems, then, that theſe precipitates are formed by an interchange of principles between carbonate of magneſia and muriate of lime, and conſiſt of carbonate of lime and muriate of magneſia. The liquor, by evaporation, depoſits a cryſtalline matter, which efferveſces with acid, and probably is compoſed of the ſame ſalts in different proportions.

Laſtly, put ſome of the precipitate in the nitric or muriatic acids. It diſſolves ſlowly, with a gradual extrication of air bubbles, and adding acid enough, it may be wholly con-

verted into deliquefcent falts; but a good deal of acid is neceffary for this purpofe. Even diftilled vinegar has this effect in part.

I intended to repeat thefe experiments with greater care and exactnefs, with a view to the improvement of the analyfis of mineral waters; but having been diftracted by other occupations, I beg your acceptance of them for the ufe of your valuable Journal, if you deem them of fufficient confequence. Before I conclude, I will take the opportunity of correcting another error which, I believe, pervades the moft recent chemical works. It is this:—Ammonia is faid to decompofe fulphate of magnefia only in part: but this, alfo, is owing to the ammonia not having been wholly deprived of carbonic acid. I precipitated the whole of the magnefia from fulphate of magnefia, by ufing ammoniacal gas in excefs. I then filtered the liquor, and boiled it with a fixed alkali; but I only obtained a little lime, difcoloured feemingly with oxide of iron.

I am, SIR,

With great refpect,

Your obedient fervant,

Warwick, May 23, 1801. W. LAMBE.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Gratuitous Analyfis of Minerals, by the Britifh Mineralogical Society.

I HAVE received the following addrefs by poft, which I give without abridgement or alteration.—W. N.

The immenfe importance which, at all times, and particularly at prefent, is attached to improvements in agriculture, and the liberal encouragement to experiments in the art of cultivating the land offered by the patriotic views of the Board of Agriculture, have rendered it a defirable object to affift the inquiries of the farmer in that part of experimental chemiftry which determines the nature of foils by an analyfis of their feveral contents.

The Britifh Mineralogical Society, having of late increafed the number of their members, find themfelves enabled to extend the plan of their inftitution, from the analyfis of *minerals*, in the ufual fenfe of the term, to that of the various foils which are made the bafis of agricultural operations.

They therefore give this public notice that they will examine, free of expence, all fpecimens of earths or foils, with a view of determining the nature and proportion of their different contents, with as much accuracy as fhall feem requifite.

They

They require, however, that the perfons fending fpecimens fhall comply with the following conditions:

That the fpecimen be about three or four pounds in weight, inclofed in a deal box properly packed with hay, and along with it an account be added of the parifh and county from which it is procured, the name of the fender and his addrefs per poft, and an anfwer to the following queries——What is the depth of the foil?—What the nature of the fub-foil?—is it on a hill or level ground? and, if the former, what is the afpect?—how near to any river or ftream?—and is the foil itfelf dry or fpringy?—what is the rotation of crops which it ufually bears?—has it ever been limed or had any other earth laid on?——Any other particulars worthy of notice may alfo be added, and a fpecimen of the fub-foil fhould all be inclofed.

Before the fpecimen is packed up, it fhould be gently dried, either in the fun or in any warm place, for a few days, but muft on no account be heated over a fire.

The box containing the fpecimens muft be fent *carriage paid* to Mr. W. H. Pepys, Jun. No. 24, Poultry, London.

The fociety intend, if confiftent with their other occupations, to return the refult of the analyfis within two months of the arrival of the fpecimen.

London, April 16, 1801.

Four Effays on Practical Mechanics; the firft on Water Wheels; the fecond on the Steam Engine; the third on Mills; and the fourth on the Simplification of Machinery. By Thomas Fenwick, Coal Viewer. 8vo. p. 84. Newcaftle upon Tyne, printed for the Author, and fold by Mawman, London.

THE particular advantage of this treatife is, that it is written with fuch plainnefs that the inftructions it contains may be carried into effect by thofe whofe acquifitions extend no farther than the firft rules of common and decimal arithmetic; in addition to which, as the facts are all deduced from obfervations and experiments on a large fcale of actual work, all defcriptions of men may receive advantage from his data refpecting mills, fteam engines, &c.

Dr. Young's Inquiries respecting Sound

Fig. 41.

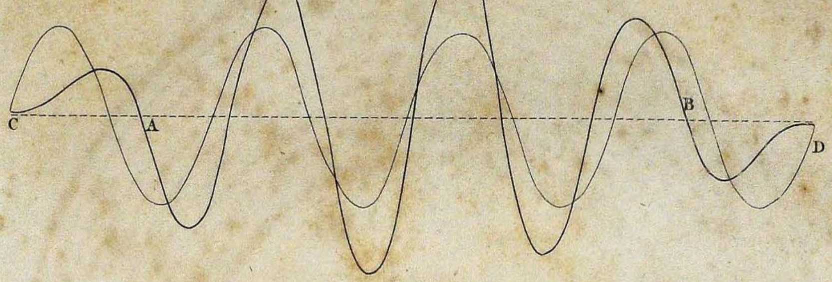


Fig. 42.

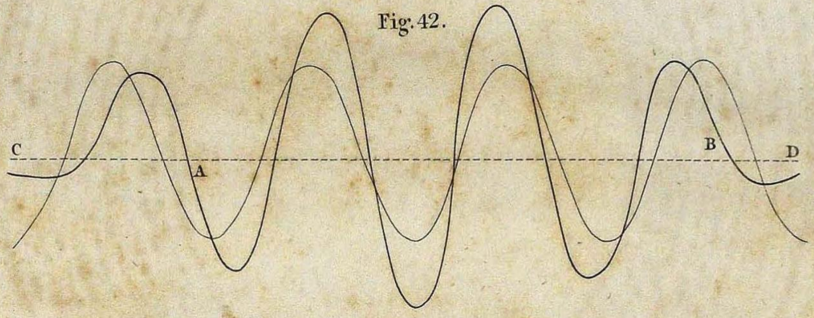


Fig. 44.

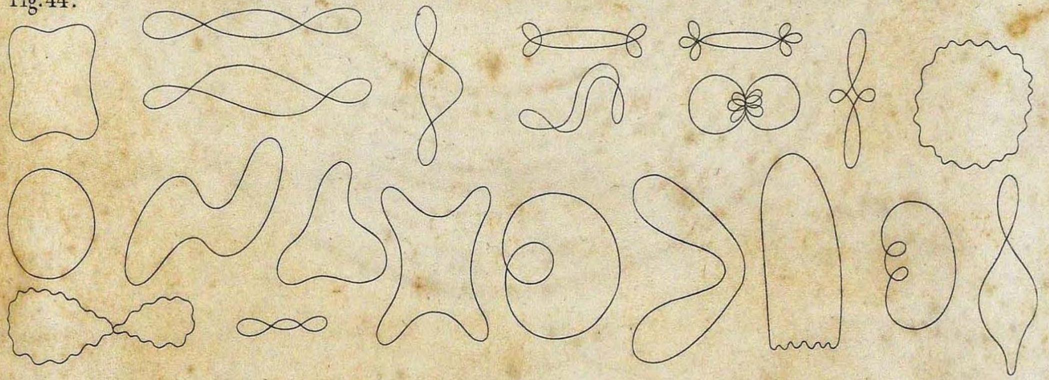


Fig. 45.

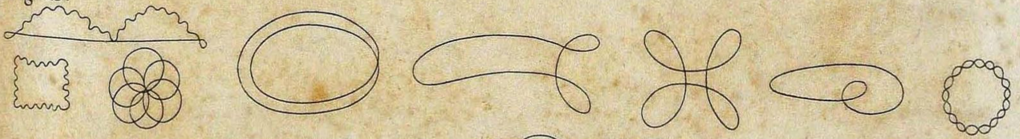


Fig. 46.

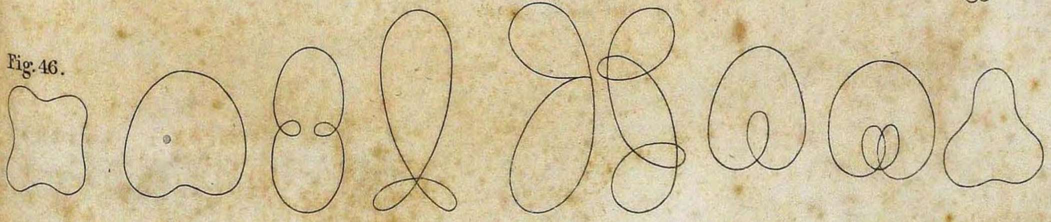
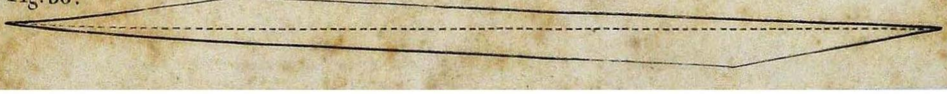


Fig. 49.

Fig. 50.



Dr. Young's Inquiries respecting Sound

Fig. 53.

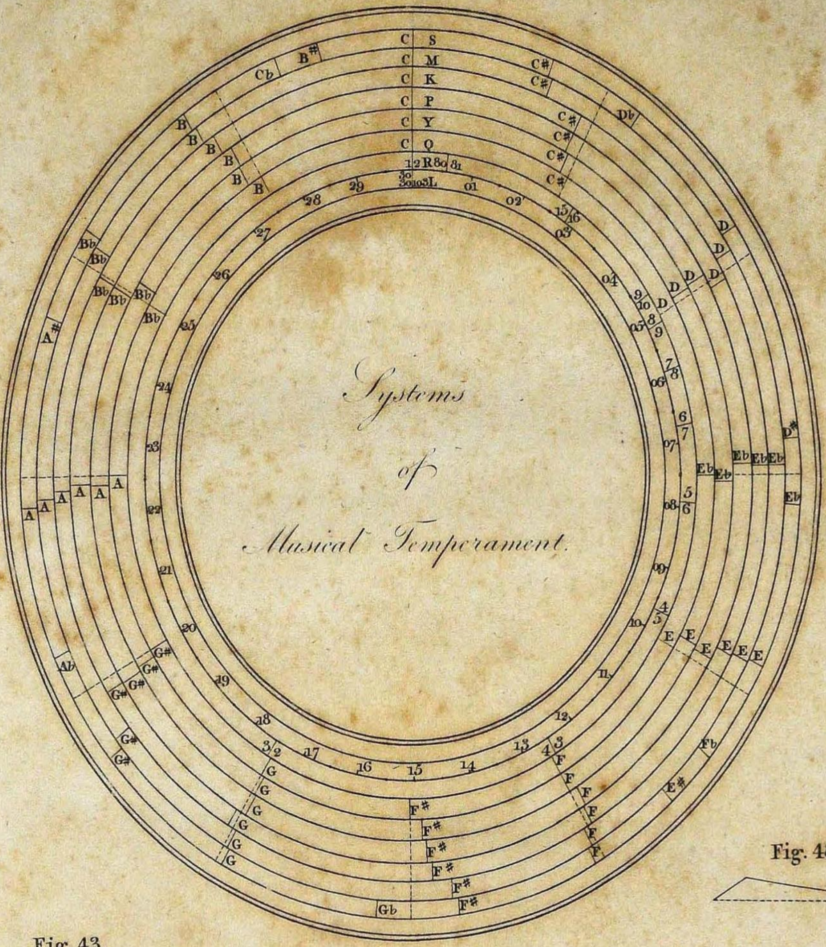


Fig. 48.

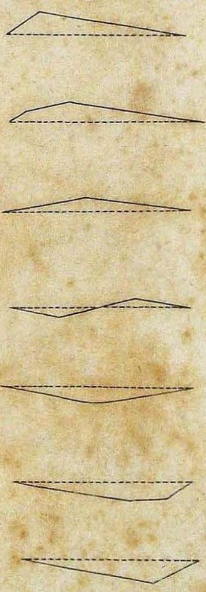


Fig. 43.

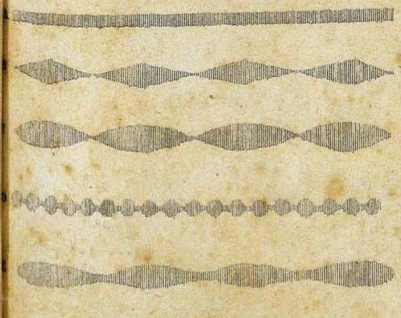


Fig. 47.

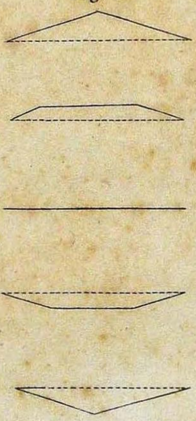


Fig. 52.

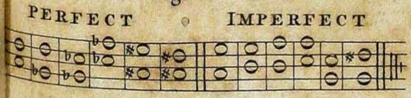
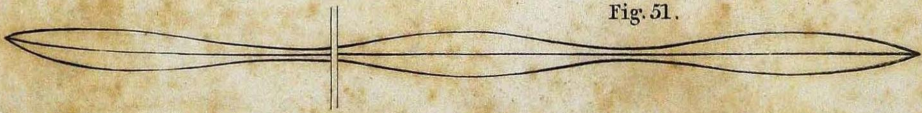



Fig. 51.